Response to Anonymous Referee #2

We thank referee #2 for the helpful suggestions in their review. We address point by point the concerns of the referee.

General Comments:

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 stucture is largely controlled by temperature, so denser (deeper) water-masses outcrop at colder temperatures (higher latitudes) 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 relatinoships 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.

We will cite Nozaki's 2001 seminal work where we talk about classification of elements into different categories but note that Nozaki's primary focus was vertical distributions whereas ours is horizontal distributions. We agree that the basic hypothesis is uncontroversial to some degree but it has not previously been subjected to statistical testing or to a comprehensive examination across such a range of elements, because this has only now started to become possible thanks to the emerging datasets from the GLODAP and most recently the GEOTRACES syntheses. We agree that the mechanisms behind the latitude-concentration relationship need to be discussed earlier. We thank the referee for their suggestion to examine relationships with physical parameters such as temperature and density and agree that doing so will considerably enhance the interest and usefulness of our study.

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").

We are obviously aware of this process but omitted to include it.

Specific 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.

We agree to add correlations to these physical parameters in order to provide insights into the drivers of the correlations between latitude and elemental concentrations.

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).

As noted in our response to reviewer #1, we make a distinction between patterns that are suspected on the basis of limited data, and patterns that are understood on the basis of datasets of a sufficient size to define the global distribution. However, we accept that we could have done a better job in citing previous studies, for instance papers arising from earlier surveys such as GEOSECS and TTO.

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 latitudinal pattern which is the focus of this discussion doesnt depend on the difference between nano- and pico-molar accuracy.

We accept this point.

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?)

We see the reviewer's point and should have cited some earlier work but we emphasise again the difference between a compelling argument based on analysis of a comprehensive dataset, in contrast to a plausible suggestion based on limited data. Earlier investigators simply did not have access to the large datasets that have recently become available.

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.

Yes.

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

Yes.

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 isnt it brought into this discussion/introduction?

The point is to acknowledge the recent work on nickel, especially because measured along an Atlantic-long transect. As mentioned above, we will cite Nozaki's 2001 seminal work where we talk about classification of elements into different categories but note that Nozaki's primary focus was vertical distributions whereas ours is horizontal distributions.

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

This could be done.

Line 80-85:a 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. doesnt 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 redifining the meaning of "bioutilized" to to support your hypothesis - because its 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*

As mentioned in the response to reviewer #1, we agree that bio(un)utilised is slightly clumsy and will replace with nutrient-like for those elements we had previously stated as bioutilised and non-nutrient-like for what we had called biounutilised. This is better but also imperfect (iron is a nutrient). Most important is that we define our use of the term clearly and unambiguously. We already do this.

References (not stated in MS)

Key, R. M., Kozyr, A., Sabine, C. L., Lee, K., Wanninkhof, R., Bullister, J. L., Feely, R. A., Millero, F. J., Mordy, C. and Peng, T.-H.: A global ocean carbon climatology: Results from Global Data Analysis Project (GLODAP), Global Biogeochem. Cy. 18, doi:10.1029/2004GB002247, 2004.

Nozaki, Y.: 2001. Elemental Distribution in Encyclopedia of Ocean Sciences, edited by: Steele, J. H., Academic Press, 840-845, 2001.