

Biogeosciences Discuss., referee comment RC3
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Comment on bg-2021-42

J.K.B. Bishop (Referee)

Referee comment on "Early winter barium excess in the southern Indian Ocean as an annual remineralisation proxy (GEOTRACES GIPr07 cruise)" by Natasha René van Horsten et al., Biogeosciences Discuss., <https://doi.org/10.5194/bg-2021-42-RC3>, 2021

Review. van Horsten et al. "Early winter 1 barium excess in the Southern Indian Ocean as an annual remineralisation proxy".

The authors describe particulate Barium, O_2 , and potential density profile data from 7 stations in the Southern Ocean along from 59°S to 41°S crossing the Antarctic polar front (51°S) along 30°E south of Africa during GEOTRACES GIPr07 (in early wintertime conditions, June 28-July 13). This is a hard to get and interesting data set. The hypothesis is that since particulate barium should have only a short residence time (days to weeks) in the water column the inventory of particulate Ba would be far lower at times of low productivity that at other times of the year. The authors report particulate Ba concentrations as high as seen in other seasons and infer an active biological carbon pump year-round. The stocks are regressed against annual mean primary production. Comparisons are made with other data sets from the Southern Ocean.

What I like about the work is the heroic effort to achieve sampling in the wintertime and the excellent primary data arising from the expedition. Also, the goal of finding the correct transfer function relating the inventory of particulate barium in the mesopelagic (an indicator of export) to remotely sensed biomass or primary productivity would be a big plus.

That said, the paper falls short of its goals. The regressions in Figure 3, and manuscript discussion provide no insight. The data south of the polar front are aliased by cloud obscured retrievals of surface chlorophyll and primary productivity (See e.g., Ocean color monthly composites) and fall on a different slope than north of the PFZ. The STZ station is an outlier. The discussion does not sufficiently unify these divergent observations.

Reflecting on other reviewer comments, I am convinced that a more comprehensive analysis of the data (now abundant) from multiple projects need to be considered. I am

sure that everyone referenced would have data to share. I echo a need for a fuller hydrographic and dissolved phase framework for data interpretation – the supplemental data are very sparse. There are some issues: (1) methodology: bottle sampling and the missing large particle fraction, and (2) the hypothesis of expected low wintertime concentrations is premised on particle sinking rates that are far too high (50 m d^{-1}) for the micron sized particles that comprise the bulk of suspended barite (sinking speed $\sim 0.1 \text{ m d}^{-1}$).

A couple of issues further complicating review is simply the lack of any access to the more complete data sets from the cruise beyond those used in figure 2 or the partially complete data sets used in figure 3. The cruise data should be available as supplemental data and also submitted to the GEOTRACES archives and DOI traceable.

I think the fundamental logic flaw (see comments below) lies on page 12 in the discussion of inferred barite residence times in the mesopelagic. I don't see a major advance beyond referenced work and don't support publication of this paper with its present interpretive framework. I encourage the Authors to look again at the results in a larger framework.

Figure 4 is not needed. It is out of place. There is room for more figures....

Some detailed comments follow.

P4 line 08, $R^2=0.83$... This seems like a bad validation of the O2 results.

p 4 line 10. The Python language did not exist in 1982. Please fix sentence.

P 5. Lines 18-20. I assume this was an in-line filter, directly connected to the side spigot of the bottle. State what was done. Also, state whether or not the large sinking particle fraction would be sampled.

p 5. line 29. If varying volumes of water were filtered, the blank will not be a constant value. $(Ba(\text{filter}) - \text{blank}(\text{filter})) / \text{volume filtered}$. to get Ba and error should be the s.d/filter blanks / volume filtered. Or is this what you did? I think the calculation was done correctly as error bars vary in size. Please clarify methods.

ALSO please state the assumed particle size fraction that has been sampled. There is no evidence that bottles adequately sample the sinking particle fraction.

P 6 Line 63. "The data..." Which data?

P9. Fig. 2: O2 scale too compressed to be useful. Authors should provide complete data as supplemental (not just pAl, pBa, Baxs) and submit as soon as possible to GEOTRACES. Include T, S, sigma theta, o2, nutrients, dissolved Ba...

p10... .Lines 31-33. "When taking into account...". There is something wrong with this sentence.

p 10. Line 34. Very high values can be associated with Rhizosolenia blooms (Bishop, 1988).

P 12. Lines 84 & 85. Line 94-95... "Residence time of barite in mesopelagic days to weeks. & Particle sinking speeds of 50 m d^{-1} ". The large particles comprising the flux do sink that fast; however, the subsurface barite is produced by fragmentation of these particles as they sink. The resulting micron sized barites sink at 0.1 m d^{-1} . Thus, the premise of decay to background on the time scale of days to weeks is invalid. Barites in the mesopelagic would have a residence time (by sinking) of hundreds of days – if not years. The sink for these barites is dissolution and reaggregation. As grazing is reduced in the wintertime then dissolution and sinking would dominate.

I've not addressed the detailed discussion further as this point and invalidates the key conclusion of the authors.

jim Bishop (UC Berkeley).

p.s. have a look at Bishop 1989 (attached - since it may be hard to find). The mapped representation of barite stocks is virtually the same as sampled here.

Please also note the supplement to this comment:

<https://bg.copernicus.org/preprints/bg-2021-42/bg-2021-42-RC3-supplement.pdf>