

Interactive comment on “Phytoplankton distribution and nitrogen dynamics in the Southwest Indian subtropical gyre and Southern Ocean Waters” by S. J. Thomalla et al.

Anonymous Referee #1

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Review of Phytoplankton distribution and nitrogen dynamics in the Southwest Indian subtropical gyre and Southern Ocean waters by Thomalla, S.J., H.N. Waldron, M.I. Lucas, J.F. Read, I.J. Ansorge and E. Pakhomov.

This manuscript details nitrogen uptake measurements, size fractionated chlorophyll and nutrient concentration data from two transects within the SW Indian Ocean across the southern subtropical front and into subantarctic waters. As this is a remote and poorly sampled region of the ocean there is merit in seeing the data published. The fact that the expedition crossed different hydrological regimes provides a great opportunity to explore the mechanisms that control nitrogen cycling and biological production

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in this region. However, there are a number of problems with the manuscript as it currently stands and I cannot recommend publication of this manuscript without major and substantial changes.

Amongst the weaknesses of this manuscript I found it to be rather convoluted or repetitive in places and unnecessarily long, making it hard to identify the major conclusions of this study. Also, the aim of the study is not clearly stated in the introduction, and it is thus very difficult to grasp the idea behind it. Also, the way the paper is structured should be mentioned to make it flow over the general idea. Some parts of the introduction, results and discussion seem to have as an aim, explaining every single detail or feature found in the data or literature, and there does not seem to be a clear question or set of questions that are being addressed. There are several inconsistencies that generate considerable confusion, such as the depth samples were collected from (to 150m or the 0.1% light level) and how appropriate the comparisons really are. I urge the authors to consider the following carefully if they choose to proceed with a revised version of this manuscript.

Major comments 1) There is much confusion over whether samples and/or calculated integrals were collected at 6 fixed depths to 150 m or 6 variable depths to the 0.1% light level (across a varying euphotic zone). This makes it quite unclear if many of the comparisons are appropriate. For example table 3 shows the 0.1% light depth to be located at 150m at all stations but figure 7 clearly shows the euphotic zone varies and is shallower than 150 m at several stations. Section 3.4 states chl-a was integrated to 150m, yet section 3.6 discusses nutrient uptake rates to the 0.1% or 1% light levels. This inconsistency must be addressed.

2) The manuscript should be significantly shortened and clarity improved. There is far too much discussion of the literature and data within the results section, much of this can be omitted, or if retained should be moved to the discussion section. The discussion is also rather long and convoluted. I suggest efforts be made to shorten the discussion.

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3) There is far too much repetition in places (such as the chl-a data which appears in section 3.4 and is then revisited in section 3.6) which could be removed by carefully restructuring the manuscript.

4) The role of iron is highly relevant but the overly long discussion of this point, without supporting data, tends to smother the actual uptake data presented here and sections often read like a literature review. The authors should refocus the manuscript to lessen the bias and emphasize the relevance of iron on the data they are presenting.

5) The sensitivity of the nutrient techniques is not discussed and may be critical in the low nutrient environment of the subtropical Indian Ocean. The method for measuring NH₄ may not be sufficient to resolve in-situ concentrations and may distort the importance of NH₄ uptake as a result if ambient NH₄ concentrations are overestimated. I suggest a short sentence on analytical sensitivities of the techniques be added.

6) The lack of isotopic dilution corrections, I suspect, cannot be addressed but there is no discussion of the impact of this omission on the uptake rates or on the interpretation of the data. A section addressing this must be added to the discussion given the overwhelming indication of the importance of regenerated nutrients. If anything this will drive f-ratios to even lower values, which then further raises questions over the sensitivities of the analytical techniques used to measure NH₄ and urea concentrations.

Minor comments 1) Page 1349 Line 8 should be redrafted for clarity (Subtropical waters were. ...), are the low concentrations Carbon or Chl-a? Given that it is the abstract, what does the >87% mean, relative to what?

2) Page 1349 Line 11 - Si(OH)₄ not SiO₄

3) Page 1349 Line 15 - ...total chlorophyll increased (<0.74 mg m⁻³)... Increased relative to what value?

4) Page 1349 Line 16 – greater than sign. Should this be less than? Or, why not writing a range of 'low' values?

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5) Page 1349 Lines 19 – 22. This sentence needs to be redrafted to make it clearer. Are the percentages indicative of the changes in microplankton cell abundances (which incidentally were not measured directly in this study so technically this sentence should reflect this) or indicative of changes in new production? Also, the word 'and' appears twice on line 19-20.

6) Page 1350 Line 2 – I suggest this reads 'provides a link' rather than 'provides the link' as you do not consider the solubility pump.

7) Page 1350 Line 6 – This estimate of the carbon pump seems low. Suggest you check Field et al

8) Page 1350 Paragraph 2 – Redraft as it is rather confusing, particularly the change from discussing the infrequent sampling of the SW Indian Ocean to discussing various techniques for measuring export. fluxes Many of the studies do not come from the SW Indian Ocean and the relevance is not immediately obvious.

9) Page 1350 line 24 to Page 1351 Line 16 - It reads more like a review and could possible be of more use in the discussion section.

10) Page 1350 lines 22 – 25 – higher, faster, lower improved. . . These adjectives are relative to, what?

11) Page 1350 – I consider the last paragraph should clearly state the aim/hypothesis of the study.

12) Page 1350 Lines 16-17 – Why is the 'median' important here?

13) Page 1353 Line 20 – Replace was with 'were'.

14) Page 1353 Line 26 – The reference to Table 1 should be removed and the stations instead shown in Figure 1.

15) Page 1354 Section 2.1 – Were all samples collected at the same fixed depths to 150 m or at varying depths as determined by the irradiance levels? It is unclear.

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- 16) Page 1354 Line 23 - SI unit for volume is 'L' not 'l' (2 L instead of 2 l).
- 17) Page 1355 Line 1 – If NO₃ was measured later in the lab, how the spikes at 10% of ambient concentrations were worked out?
- 18) Page 1355 Line 4 – Were deep samples also flushed with surface seawater? What impact did the temperature differential have on uptake rates?
- 19) Page 1356 Line 6 – It is not possible to see the eddy the authors refer to nor the eastward movement of the eddy discussed later (Page 1357). I suggest reference to the eddy be removed.
- 20) Page 1357 Paragraph 1 – There is no way you can see the isopycnals referred to on a section of temperature. I suggest this paragraph be removed or a section of density added.
- 21) Page 1357 Lines 4-9 - How was the flow observed? Are there any velocity measurements? Different latitudes are mentioned when referring to characteristics in the water column structure, but there are very few Latitude tick marks in the figures and thus it is rather hard to follow descriptions.
- 22) Page 1357 Line 10 to 21 – There is a lot discussion here of features that cannot be seen in the figures. The authors should consider a TS plot, particularly as reference is made to water masses on Page 1358.
- 23) Page 1357 Line 13 – seasonal or permanent thermocline?
- 24) Page 1357 Lines 15-16 – Where's the evidence for the Ekman layer driven from the subantarctic zone, how was this estimated?
- 25) Page 1357 Lines 19-21 – Where can we see the TS relation showing the mixing?
- 26) Page 1358 Paragraph 1 – Too much discussion. Move this to the discussion section.

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- 27) Page 1658 Lines 3-5 – What depth range? What is warm and what is cold? It looks colder southwards.
- 28) Page 1658 Lines 11-19 – This section is confusing, definitely a TS diagram would be most useful.
- 29) Page 1358 Section 3.4 – This section needs to be rewritten as it is confused and unclear. For example, there is further discussion of isopycnal surfaces which are not shown, repeated reference to the westward flow which is inferred and not shown and it is not clear if the reference to a series of higher and lower concentrations refers to surface values or to vertical concentrations. Why were chl-a concentrations integrated to 150 m when the euphotic zone was often shallower? Do you mean the euphotic zone?
- 30) Page 1359 Line 3 – Here potential temperature is mentioned. Is the contour plot showing temperature or potential temperature? In either case, this should be clearly stated and used consistently.
- 31) Page 1359 Line 7 – Reference to Figure 4b should be moved to the end of the sentence.
- 32) Page 1359 Line 17-19 – Where can the distribution to greater than 100 m can be seen?
- 33) Page 1359 Line 23 – It is clear from Table 3 that nitrate concentrations in the subtropical waters were $\ll 1$ mmol m⁻³ and very possibly in the nanomolar range. A more appropriate estimate of the subtropical concentration should be presented.
- 34) Why is the SF chl data not discussed within section 3.4? These two sections should be combined to improve the clarity and structure of the manuscript.
- 35) Page 1359-1360 – Why is Si not described in more detail? The contour plot shows rather dubious data. Can you describe why it looks so different than nitrate?

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- 36) Page 1360 Line 6 – What exactly is meant by ‘nutrient’ and ‘N’ uptake? Is it N-nutrients and total N uptake?
- 37) Page 1360 Line 14 – What do you refer to as the ‘total nitrogen pool’?
- 38) Page 1360 Lines 21-23 – The distribution of urea doesn’t seem to show a gradual decrease southwards, to me it looks more like NH₄, that is, it does not show any clear spatial pattern.
- 39) Section 3.6.2 – What is the point of this paragraph?
- 40) Section 3.6.3 – Is it total integrated chlorophyll?
- 41) Section 3.6.4 – The uptake data is presented both in Figure 9 and Table 3. I suggest omitting Figure 9 as it is barely discussed after its introduction on Page 1362.
- 42) Should not the symbol used to represent integrated uptake rates ($\rho \int N$), be $\int \rho N$?
- 43) Page 1362 Line 17-19 – With the integrated values that would seem to be the case, however chl-a and uptake are higher close to the surface, while nitrate concentrations are high at depth.
- 44) Page 1363 Lines 1-2 – I disagree with this statement, the ratio ‘kind of’ increases with depth. Besides, in the figure, as it is, f-ratio symbols do not match nitrate concentration symbols in terms of positions at depth.
- 45) Page 1363 Line 4 – According to table 3 there are no uptake measurements at 15 m.
- 46) Page 1363 Lines 5-8 – I’m not sure about this statement. NP4 looks like NP1 and NP2. It is difficult to appreciate with such small graphs, you could always restrict the scale to 0.5 or to the maximum value.
- 47) Page 1363 Line 10 – In your plots it looks more like at 120 m depth.

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- 48) Page 1363 Lines 11-14 – How come you integrated the f-ratio? Did you work the ratio out using integrated uptake rates? Or did you actually integrate the ratio? If the second case were true, well, that would be an odd thing to do and you would end up with units actually.
- 49) Page 1363 Line 23 – “generally exceedingly low” should be replaced.
- 50) Section 4.1 – Some of this is repeated from the results section, which is not necessary. There seems to be a slight confusion over the seasonal and permanent thermocline. The suggestion that Fe replete conditions exist north of the STF and in the subtropical waters requires clarification – what are the sources/concentrations/evidence for this statement?
- 51) Page 1364 Line 28 – The statement regarding elevated ambient nitrate at depth due to diffusive flux is slightly misleading, could it not simply be due to sampling within the thermocline? How do you identify the diffusive flux? The increase in the f-ratio with depth might be due to an increase in NO₃ (this can be confirmed from the data) but could it not also be due to a relative reduction in NH₄ or urea?
- 52) Page 1366 Line 9 – Again, if potential temperature is used, this should be clearly indicated when first mentioned and also in the figures and figure captions.
- 53) Page 1366 Line 17 – f-ratios is figure 12.
- 54) Page 1366 Lines 24-26 – Where is the 72 m average depth of the euphotic layer coming from? By looking at table 3 it would seem 150 m. By other hand, by looking at figure 7, the depth of the 0.1% light level is so variable and within a large range, that averaging it would not be representative.
- 55) Page 1367 Line 4 – Nitrate uptake is an ‘energetically’... What?
- 56) Section 4.2.2 – The RPI is a discredited and unreliable indicator of nutrient preference due to the impact variations in ambient nutrient concentration have on the calculated RPI value. I suggest reference to the RPI be removed from the manuscript.

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- 57) Page 1368 Line 3 – Nitrogen or Nitrate uptake?
- 58) Page 1368 Line 26 – ‘is also to be expected’ sounds odd, why not, ‘would also be expected?’ By the way, why?
- 59) Page 1369 Line 10 – Add a suitable reference to support the claim regarding biogenic silicate deposition.
- 60) Page 1370 Lines 2-6 – Yes, but you also said the nitrate uptake needs light and nitrate has been depleted at the surface, so integrated nitrate does not really convey this information.
- 61) Page 1370 Line 13 – The reference to the diffusive nitrate flux is misleading. Could the increase in the f-ratio in the AF region not simply be due to increased nitrate concentrations rather than due to an active supply mechanism? Otherwise, where is the observed diffusive nitrate flux? Numbers?
- 62) Page 1370 Line 26 – If you integrate chl normalized uptake you do not end up with the units you show.
- 63) Page 1371 Line 15 – The two mechanisms most likely. . .
- 64) Page 1371 Line 19 – Does it actually explain the observed increase in chl, or it would explain?
- 65) Page 1372 Line 16 – Is it 42.5 or 43.5 deg South, or do you mean, now at 43.5 deg South?
- 66) Page 1372 Line 18 – Integrated chl?
- 67) Page 1372 Line 25 – ..in the euphotic surface layer of . . .
- 68) Page 1373 Line 18 – What is meant by rNH₄? Or, is it that the ‘r’ was not converted to the Greek symbol?
- 69) Conclusions Page 1374 Line 15 – Do you mean South Atlantic or SW Indian? Line

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22-25 – This sentence appears out of nowhere, and discusses issues which are not really present in the manuscript (POC export, respiratory CO₂ losses, biological CO₂ drawdown). I suggest this sentence be removed.

70) Page 1375 Line 1 – How do you know it was maintained by turbulent mixing and large scale upwelling? Or do you mean, was ‘likely’ maintained?

71) The reference list requires attention as several of the references were presented out of order, notably the 2 Falkowski references appearing between Hoffmann and Honjo.

72) Page 1376 Line 32 – Jickells.

73) Page 1380 Line 9 – Iron deficiency. . .

74) Page 1381 Line 10 – Is it not The fourth cruise?

75) Page 1382 Line 12 – Equatorial Pacific.

Tables 1) Table 1 serves no purpose and the relevant information it contains (station positions) can easily be incorporated into a revised version of Table 3, or omitted altogether (see comment regarding Figure 1)

2) Table 2 is also unnecessary as much of the information (max chl-a concentrations) appears in the text or figures already. The contribution the various size fractions make to the total chl-a is important but is it appropriate to show results to 150 m as the euphotic zone was generally shallower than this at most of the stations sampled? It is also unclear if the SF values represent the percentages at the same position as the max chl-a concentration or average contributions in the two regions.

3) Table 3 contains the real results of this study, but not only repeats data that is shown in Figures 7 and 9 it is also misleading. Were samples collected at fixed depths as seems to be the case from the depth column or were samples collected from light depths (which vary with latitude) as shown in Figure 9. Table 3 gives the misleading

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impression that the 0.1% light depth was at 150 m at all 6 stations. Also, a couple of values seem to be missing.

The presentation of NH₄ and urea uptake rates with up to 9 decimal places is overkill and unnecessary.

Figures In general the figures were of sufficient clarity but the number of figures could easily be reduced by either combining some figures or by omitting them altogether.

1) I suggest a new version of Figure 1 be created which includes the Prince Edward islands, labels on the major landmasses, better bathymetric contours and the approximate positions of the fronts and currents discussed in the text. It should also include markers to identify the position of the stations NP1 – NP6, thus removing the need for Table 1.

2) Figure 2. Is it temperature or potential temperature? It is very difficult to appreciate the variability of the size-fractionated chlorophyll. You could always include a break point, say between 0.3 and 0.8 and slightly squeeze the three higher values toward the upper part of the plot.

3) Figure 3. What is the white line at about 24 degrees south? Also, make sure you have the same font size of labels in all contour plots, and less contour labels would be better since it is very distracting to see a figure with so many numbers inside.

4) In the legend of Figure 4 it states that the positions of station NP1-NP6 are indicated. I could not see them in the figure. Or, by this do you mean AF, STF and SAF.

5) Figures 2c, 2d and 4b and 4c could be combined to highlight the differences between the two transects more easily.

6) Figure 5b appears to show a major upwelling of silicate rich waters at 35S but there is no indication of a similar injection of nitrate at the same position. Why is this?

7) Figure 6. What are the numbers at the top of the bars? I know it is sort of obvious,

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but these should also be indicated in the caption of the figure. The same applies to Figures 8, 10 and 12.

8) Figure 7 clearly shows that the depth of the euphotic zone varied between stations. This raises the question of why integrals to 150 m are frequently presented. What is the justification for integrating to depths that may not be considered biologically relevant? In the caption I would suggest deleting 'Together with' and replacing this with 'Dotted lines show. . . . And dashed lines show. . . .' Why figure 7f does not show a SML? Is it deeper than the depth range cover by the plot?

9) Is Figure 9 necessary given that the data is also presented in table 3? I suggest the data either be tabulated or presented as a Figure, not both. Units used in the caption of this figure are incorrect.

10) The contents of figure 10 could be tabulated without loss of detail particularly as not all values are presented in the text

11) The nutrient concentration data shown in Figure 11 is also presented in table 3, this is not necessary. The units given in the legend (mg-at m⁻³) need to be updated. The profiles of f-ratios and some N-nutrient profiles look to be on different depth scales. For example, in Figure 11c it looks like you have urea and ammonium measurements to 100 m, yet somehow you have f-ratios to 130m. Why are the profiles of f-ratio not present in Figure 9 with the more relevant uptake rate data?

12) Are the f-ratios based on integrated uptake profiles or derived in some other manner? It is not clear.

13) Figure 14 should be omitted as the RPI has been shown to be severely biased by nutrient concentrations and the results misleading.

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