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Biogeosciences Discussions

Interactive comment on "Effects of iron on the elemental stoichiometry during EIFEX and in the diatoms *Fragilariopsis kerguelensis* and *Chaetoceros dichaeta*" *by* L. J. Hoffmann et al.

Anonymous Referee #2

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Review of Hoffmann et al 2007 Biogeosciences Discussion

This paper presents results from the EIFEX experiment that indicate that the addition of Fe led to an increase in the Si:N, Si:P and Si:C ratios in suspended particulates contrary to the results from other Fe fertilization experiments. They account for this by citing previously published observations on the shift in the diatom assemblage to more heavily silificied forms after the addition of Fe. This is a unique result that differs from other fertilization experiments making it worth publishing. The authors also explore the effect of Fe stress on the elemental composition of two Southern Ocean diatoms. Those results confirm previous observations that shifts in the diatom Si:organic matter

ratios induced by changes in Fe availability are not always due to a response of silicification and that changes in cellular organic matter content can be the main driver of shifts in Si:C, Si:N ratios. The inclusion of F. kerguelensis is ideal given its enormous important to Southern Ocean biogeochemistry. I also note that changes in particulate organic phosphorous concentrations are measured in both the field and culture experiment which is rarely done, but a vital component to examine if we are to have a complete understanding of iron effects of phytoplankton elemental ratios. Based on the novelty of these results and their relevance to the discussion of how Fe affects biogeochemical cycling in the ocean I recommend publication.

I have just a few significant scientific criticisms and some of these may simply be due to language issues.

Abstract, Line 18: I would rephrase the sentence to read “These results indicate that iron limitation does not always increase silicification in diatomsĚ”

I would recommend eliminating the last sentence of the Abstract. The variable effect of Fe on cellular Si in diatoms has been demonstrated several times going back to the work by Takeda in 1998 and the field work of Frank et al. So we have know for years that these relationships are more complicated than implied by Ed Boyle’s statement that pumping iron makes thinner diatoms. So the statement that the issue “is more complex than hitherto assumed” is not entirely accurate. Assumed by who? Certainly not me, and likely not by many others who have read the literature that has emerged since 2000. I am not trying to undermine the significance of the work presented here, but I believe that the abstract would read just fine without the last sentence.

Page 251, line 10: This paragraph is a bit inconsistent in its treatment of Fe effects. The physiological basis for an effect on N assimilation is presented, but the physiological basis for Fe effect on C quotas is not presented. I would recommend a brief treatment of both Fe effects on N and C metabolism.

Page 251, line 30: This paragraph would benefit from a brief description of the current

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hypotheses concerning how Fe affects the Si content of diatoms to match the treatment for N and C mentioned above..

Page 251, Line 23: I disagree that “It has generally been assumed” that shifts in diatiom Si:N and Si:C ratios under iron stress is due to increases in cellular Si levels. This may be a simple language issue. Takeda (1998) showed a variable response of cellular Si to Fe stress and Franck et al (2003) come to the conclusion that increases in Si:N ratios under low Fe is mainly driven by declines in cellular N rather than increases in cellular Si. I suggest this modification: “While this can be caused by decreased cellular Si levels upon release from iron stress (Takeda 1998, Hutchins & Bruland 1998) other studies show the effect to be driven mainly by increase in cellular nitrogen and carbon with little or nor change in cellular Si (Takeda 1998, Franck et al. 2003).”

Page 252, Line 20: change “compared” to “completed”

Section 4.1: I am very glad to see that POP was measured. This is a nice addition as there is very little data on this topic.

Page 260, Line 25-29. How about: ” This shift in the diatom assemblage towards more heavily silicified species may have overwhelmed any reduction in the Si content of individual cells.”

Page 262, line 8: change “it is generally accepted” to “it has been hypothesized”

Page 262: Change “As in situ iron fertilization experiments are performed with the aimĚ” to “One of the common goals of in situ iron fertilization experiments is to evaluate the effect of iron limitation on carbon export to the deep sea”. I don’t think the motivation for most iron fertilization studies was geoengineering.

Page 262, line 28: Change “stability” to “strength”

Page 264, first paragraph. The results have another implication that may be worth reiterating here. I would add that changes in the implication for these changing elemental ratios for the elemental composition of diatoms must also be examined with caution.

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