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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 #3

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The manuscript presents novel data on the effect of iron availability upon the mechanisms responsible for changes in stoichiometry of major elements in diatoms and therefore deserves publication. However my opinion is that the exploitation of data has been too fast in several points and then need to be addressed more carefully. Authors often use shortcuts which focus on the conclusions they want to reach. Also on such a subject like phytoplankton elemental stoichiometry, the literature review has to be exhaustive while, as is often observed, the literature analysis seems to me to be restricted to the recently started e-literature era (i.e. considering only works > 1995). It is very surprising that the classical paper by Brzezinski (1985) has been omitted. Also a large part of the manuscript addresses the question of vertical export, a point which

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is not supported by any new data in this paper.

My opinion is that the manuscript has to be rewritten and is not acceptable as it is. It would probably benefit from a fusion with the other companion paper submitted at the same time to Biogeoscience. One has to avoid the dispersion of experimental data between several papers and to focus on more synthetic approaches aimed at facilitating the analysis by colleagues and foster faster progress in knowledge and comprehension increase.

Important : Referring to biogenic silica, please use the acronym BSi which is widely accepted instead of introducing the confusing "bPSi" terminology.

English phrasing should be carefully checked.

Introduction

This part has to be reconsidered in more details and strengthened. My impression is that authors have been tempted to extract only the recent literature and in that amount of publications have been tempted to choose what stuck closer to their impressions. For example, it has never been argued in the past (and certainly Redfield did not) that Redfield ratios are supposed to be representative of living organisms. Rather, studies conducted in the 70's-80's tended to demonstrate that Redfield ratios are more specific of aged organic matter and are individually found in senescent populations or detritus accumulation levels. Perhaps this should be mentioned with appropriate references.

Examples of enzymatic processes presented are only examples and not exclusive of other important processes at the cellular level (chelatase, eventual nitrogenase functions, etc. . .). For example, the increase in Chl a per cell is related to resuming by Fe addition of both chlorophyll synthesis and nitrate utilization.

"Therefore in High Nutrient Low Chlorophyll (HNLC) regions like the SO, where iron limits phytoplankton growth, higher POC : PON and lower PON : POP ratios compared to the Redfield ratio may be expected." : this is far too simplistic, regarding the different

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cellular sites of Fe action and the differences in environmental factors (e.g. light/vertical mixing) among the different HNLC areas.

Material and Methods

"We additionally grew *F. kerguelensis* without iron and EDTA addition," The sentence can be confusing and I propose you be more precise, like "We additionally grew *F. kerguelensis* without iron addition and with EDTA addition,". At a first look I got the impression that no Fe nor EDTA had been added.

Please give the refs of POC, PON, POP and BSi measurements (Hoffman et al., 2006, is not a classical methodological reference).

Results

Table 2 should be replaced by a figure. If one does so, one has to consider that the in and out patch data are not that different at the end of the experiment (i.e. around day 35) although the previous evolutions are clearly differentiated. It is my opinion that out patch waters are developing a biological activity at the end of the experiment which adds more complexity about the interpretation of in patch evolution.

"In *F. kerguelensis* and *C. dictyota* cultures, iron fertilization resulted in a significant increase in maximum growth rate, chlorophyll concentrations, and photosynthetic efficiency (F_v/F_m) compared to the non fertilized treatments (Fig. 3 and Table 3)". I do not totally agree because the elemental composition at the cell level of *C. dictyota* are ambiguous as treatment B and D give more or less the same results with opposite treatments. Also why is the experiment A lacking for *C. dictyota*?

Discussion

4.1 Deviation from the Redfield ratio

"These uncertainties lead us to the suggestion to use PON : POP and POC : POP ratios with great caution in terms of nutrient drawdown ratios and for biogeochemical

modeling. 3 This is not really your results but is self-contained in the Fu et al paper.

"The general observation that the POC:PON ratio is less affected by environmental conditions and generally closer to the Redfield ratio makes it a far better environmental proxy for these purposes." I disagree with this information which arises from an incomplete review of the extensive literature on C/N/P ratios in the particulate matter (see my remark in introduction). I think that you should think about reviewing the literature between 1960 and 1995.

4.2 Impact of iron on silicification

Be careful about the "opal paradox" which has been more or less completely ruled out by several papers including the Pondaven et al paper in Nature in relation to sediment focusing processes affecting the sedimentary budgets of Si. Although I am fully convinced that phytoplankton export to depth is a major point of interest regarding Fe-artificially-induced fertilization, I disagree this subject has to be addressed in this paper as authors do not provide any data on export.

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